

The Impact of Immigrant Deportations on Latino Segregation

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In response to the rise in the number of unauthorized immigrants over the last several decades, local communities have taken unprecedented steps to facilitate immigrant detentions and deportations. Since the early 2000s, the number of annual deportations has soared, rising from roughly 150,000 in 2001 to over 400,000 in 2013. Concerns about these activities have further intensified by recent actions to target and remove non-criminal immigrants lacking authorization (Pew Research 2018). A growing body of research in economics, demography, and sociology has documented the wide-reaching impacts of these policies. Recent work has shown, for example, that local enforcement policies are associated with reductions in the non-citizen population (Kostandini et al. 2014) and Hispanic public school enrollments (Dee and Murphy 2018), and increases in long-distance moves among the unauthorized (Watson 2013). Other work has also shown that local enforcement has measurable impacts on Hispanic wages, home foreclosure, food security, physical health, and mental distress (see Kaushal 2018; Orrenius and Zavodny 2015; Potochnick, Chen, and Perreira 2016; Rugh and Hall 2016; Vargas, Sanchez, and Juárez 2017).

The impact of local enforcement on Hispanic populations, migration patterns, and socioeconomic position raises the possibility that patterns of residential segregation within these areas have been altered. This follows from arguments that deportations and growing fear among Latino families located in areas with restrictive orientations toward immigrants spur residential mobility, redistributing Hispanics across neighborhoods, and thereby altering the extent to which Hispanics and non-Hispanics reside in the same neighborhoods. These dynamics are likely to be especially pronounced if intensified enforcement policies incentivize Hispanics to further isolate themselves from non-Hispanics. The implications of these possibilities are substantial, given the long-standing recognition that the spatial distance between groups translates to the social distance between them, and that heightened segregation limits opportunities for interactions necessary for assimilation to take root (see Massey 1985; Duncan and Lieberson 1959; Rugh and Massey 2014).

In this paper we evaluate these dynamics by testing how local efforts to deport immigrants altered Hispanic residential segregation patterns in US counties. To do so, we leverage variation in the implementation of 287(g) agreements between local municipalities and the federal government during the 2000s, which gave police officers in affected areas substantial leeway to question, apprehend and detain individuals suspected of being in violation of immigration law. We link these agreements to county-level changes in patterns of residential segregation between Hispanics and other racial group members between 2000 and 2010. We are able to identify the plausibly-causal effect of enforcement on segregation using a difference-in-differences framework that compares trends in segregation for counties that

implemented 287(g) to a set of counties that applied for, but never implemented the program. To further isolate the effects, we benchmark these estimates to corresponding changes in the segregation among non-Hispanic populations (e.g., between whites and blacks), which are unlikely to be affected by enforcement policies. In addition we explore a range of possible mechanisms connecting enforcement and segregation, including documented detention events, changes in migration behaviors, and fear of engaging with federal officials.

Data and Methods

Our analytic design relies on county and municipal level agreements permitting local policing and incarceration agencies to coordinate with the Department of Homeland Security on immigration enforcement. More specifically, we leverage variation in applications to participation in the 287(g) program that was created as part of the 1996 Illegal Immigration Reform and Immigrant Responsibility Act. We rely on applications from 164 counties¹ to participate in the program, obtained via FOIA requests to the DHS. Of these applications, 53 were approved, while the others were denied by DHS or, in some cases, withdrawn.² For the approved applications, we also collect information on the dates of implementation to distinguish between early and late adopters, as well as the enforcement arrangement that was agreed to (i.e., jail-based or task-based enforcement).

To measure racial residential segregation within counties, we rely on block-level data from the 2000 and 2010 decennial censuses. These data, in contrast to units such as census tracts, give us the granularity to explore patterns of segregation in smaller and non-metropolitan counties (see Lichter et al 2010, 2016).³ We use these data to calculate residential dissimilarity, the mainstay of segregation analysis, between whites and Latinos, whites and blacks, and blacks and Latinos. In line with convention, we calculate these scores only for counties with at least 5 census blocks and that contain at least 100 members of each focal group. To avoid bias from racially-concentrated institutional settings (e.g., jails and prisons), we exclude all blocks where 25% or more of the population is institutionalized. Mean white-Hispanic dissimilarity (weighted by Hispanic population size) for counties that meet these criteria was .595 in 2000 and .568 in 2010.⁴

To explore possible mechanisms connecting local enforcement to Latino segregation, we use data on a variety of dimensions. To test arguments that enforcement activities alter the behaviors of in- and out-migrants, we use race-specific measures of county net migration based on differencing observed population counts from expected counts determined from

¹ We use the BEA's definition of counties that assigns independent municipalities in Hawaii and Virginia to encompassing counties (see <https://apps.bea.gov/regional/pdf/FIPSMODIFICATIONS.pdf>).

² Specific information on how applications were evaluated is not available, but a DHS performance report indicates that concerns over whether counties had the fiscal and resource capacity to carryout immigrant-related enforcement were relevant (see Department of Homeland Security 2010).

³ In supplemental work, we reanalyzed our models using block group data from Geolytics that normalizes geographic boundaries of these units to be constant overtime. Results from these models are substantively equivalent. Indeed, the main DD estimates based on block group data are noticeably (albeit not significantly) larger than are those reported here, based on block-level data.

⁴ The estimated trend in white-Hispanic county segregation mirrors the documented decline at the metropolitan level (see Logan and Stults 2011). The higher levels reflect the use of more-granular block level data, allowing for the detection of segregation at smaller scales (see Iceland and Steinmetz 2003)

fertility and mortality records from the National Center for Health Statistics (see Winkler et al. 2013 for a detailed description of the data). While these data do not permit the disaggregation of in and out flows, they provide comprehensive coverage of migration patterns during the 1990s and 2000s. We also consider the possibility that heightened enforcement increases the fear of engaging with government employees, thereby reducing efforts to accurately count the population and potentially biasing estimates of segregation. To do so, we use data from the Census Bureau’s post-enumeration Census Coverage Measurement Survey that provides estimates of enumeration gaps in decennial censuses for counties with populations of 100,000 or more. While these estimates are tabulated by race/ethnicity, they provide a basic assessment of the accuracy of Census counts (see Mule 2012).

We also explore several factors that may moderate the relationship between local enforcement and segregation. This includes distinctions based on the timing of 287(g) agreements given reports suggesting that counties who adopted later were more aggressive in targeting unauthorized residents, regardless of criminal offenses. Similarly, we test the extent to which enforcement strategies – whether agreements delegated responsibility to police task forces or to jail facilities – produced differential responses. We also incorporate measures of local immigrant detention activity using data on detainees placed by DHS, and obtained via FOIA by the Transactional Research Access Clearinghouse. Based on the address of the facility where the detainee was placed, we determine annual detention counts for each county. Using information on the basis for initial detention, we classify non-serious detainees as cases where no conviction was filed or where the most serious offense was a misdemeanor. To test arguments about intensity of non-criminal enforcement efforts, we take the ratio of non-serious and serious detainees.

Lastly, we incorporate a range of statistical controls to adjust for any pre-existing differences among applicant counties. These include percent Hispanic, percent foreign-born, natural log of median household income, Hispanic household size, and the percentage of Hispanic workers who commute to work using means other than driving alone. In ongoing work, we also incorporate measures on the size of local unauthorized populations using data derived by imputing legal status in the Census public-use microdata samples based on known correlations from the Survey of Income and Program Participation, with inflation adjustments made to reach control totals equal to estimates from the Department of Homeland Security (see Bachmeier, Van Hook, and Bean 2014). We link the PUMA-level estimates this procedure produces to counties using spatial allocation tools that nest PUMAs within or assign PUMAs to counties where spatial correspondence is high.⁵

Our analytic approach uses a difference in differences (DD) specification, that follows the generic form:

$$Seg_{jkct} = \gamma Post_t + \delta 287_c * Post_t + \alpha_c + \beta X_{ct} + \epsilon_{ct}$$

where Seg_{ct} is residential dissimilarity between groups j and k ; $Post_t$ is a dummy indicating the end of the decade (2010); 287_c is an indicator of whether the county implemented a 287(g) agreement; α_c represents county fixed effects; and X is a vector of basic controls. Our analytic

⁵ We use a spatial allocation factor of 80%; meaning that we only retain data for PUMAs where 80% of the population resides in a single county. This approach produces estimates for 510 counties.

interest lies in δ , which is the DD estimate of the extent to which changes in segregation during the 2000s were different for counties that implemented 287(g) compared to those that applied, but did not implement the program.

Preliminary Results

To date, we have fully assembled the county data file including all of the variables described in the preceding section. A visual representation of the relationship between 287(g) and Hispanic-white segregation is shown in Figure P1, which plots dissimilarity scores in 2000 and 2010 for all counties (grey circles), counties that applied but did not implement 287(g) (green circles), and counties that implemented 287(g) (pink circles). The figure suggests that declines in Hispanic-white segregation during the 2000s (all dots below the orange identity line) were less pronounced in implemented counties than in non-implemented (rejected) areas.

Results from the DD models are shown in Table P1. The first column simply formalizes the visual depiction in Figure P1, indicating that changes in Hispanic-white dissimilarity were significantly higher in counties that implemented 287(g) than in rejected counties. The impact is fairly substantial, with the estimate implying that Hispanic-white segregation is about 2.3 points higher in 287(g) areas; or, alternatively, that declines in Hispanic-white segregation were about 60% smaller in 287(g) counties than in rejected areas ($[\text{.023}/\text{-.058}]+1$). Column 2 incorporates controls for county demographic, income, and commuting behaviors, which have no meaningful impact on the 287(g) effect. The remaining models shown in Table P1 report results from corresponding models predicting black-white (columns 3-4) Hispanic-black (5-6) segregation. Consistent with expectations, the effects of 287(g) on both outcomes are null, implying that local enforcement did not alter these segregation patterns.

Two types of heterogeneous treatment effects are explored in Table P2: variation in the timing of implementation and the type of agreement signed with DHS. In line with our expectation that later agreements were more aggressive in nature and, specifically, in targeting non-serious criminals, the results from Model 1 suggest that the impact of 287(g) was most pronounced in counties adopting the policy in the latter part of the 2000s (after 2007). While we cannot reject the null hypothesis that the two coefficients are equivalent, the DD estimate for late adopters is noticeably larger (and statistically significant) than the non-significant estimate for early adopters. We similarly, explore the impact of agreement type in Model 2. The results suggest little difference in 287(g) between counties that adopted jail, task force, and joint (hybrid) agreements. While inconsistent with expectations, it partly follows from qualitative work suggesting that variation in county's intensity in administering 287(g) does not map well to the types of agreements that were signed (Capps et al. 2011).

Lastly, we present initial work exploring two of the plausible mechanisms connecting local enforcement and segregation in Table P3. Specifically, we test arguments related to the roles of internal migration and Census undercoverage due to fear of interacting with federal officials. In both cases, we first run DD models regressing the mechanisms against 287(g) status, and then run a model of Hispanic-white segregation including both measures. The estimates in the first column show the impact of 287(g) on Hispanic net migration rates (expressed as net migrants per 1,000 expected population) and indicate that implementation of the enforcement agreement was associated with reductions in rates of Hispanic net migration, but not

significantly so. The second column reports estimates for white net migration, showing no impact of 287(g). Estimates in the third column indicate that Census coverage was no different in implemented than rejected counties; indeed the point estimate suggests that Census coverage may have been slightly better in 287(g) areas, although again, the coefficient is not statistically different from zero. Lastly, we include these measures as covariates in our model of Hispanic-white segregation. While Hispanic net migration is associated with segregation, the inclusion of neither measure alters the impact of 287(g) implementation on Hispanic segregation in a meaningful way.

Ongoing Efforts

Over the next several months, we will continue to explore the mechanisms linking immigration enforcement to residential segregation patterns. Most importantly, we will use the detainer data that we recently acquired to better test arguments about how treatment intensity – and, specifically, how immigrant detentions – shapes segregation patterns. We also intend to explore patterns of neighborhood change within counties; assessing the extent to which census blocks in 287(g) counties with varying racial compositions – e.g., majority-Latino, integrated white and Latino – undergo unique patterns of change. We are completely confident that we will have the paper ready for discussion well in advance of the PAA meetings.

Figure P1: Hispanic-White Segregation, 2000 to 2010, by 287(g) application

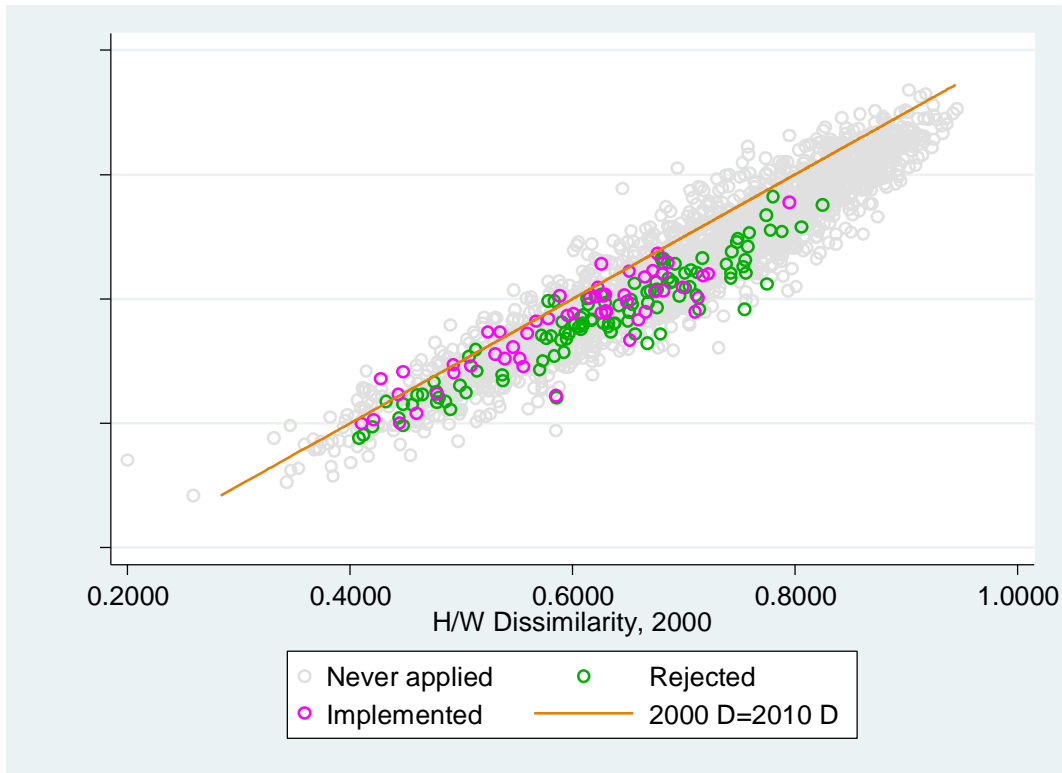


Table P1: Estimated Effect of 287(g) on Segregation

	Hispanic-white <i>D</i>		Black-white <i>D</i>		Hispanic-black <i>D</i>	
	(1)	(2)	(3)	(4)	(5)	(6)
Implemented 287(g)	.023 ** (.006)	.020 ** (.006)	.007 (.006)	.005 (.006)	.009 (.008)	.009 (.008)
Post period (2010)	-.058 ** (.003)	-.064 ** (.010)	-.051 ** (.004)	-.042 ** (.011)	-.066 ** (.004)	-.071 ** (.014)
County fixed effects	yes	yes	yes	yes	yes	yes
Controls	no	yes	no	yes	no	yes
N of applicant counties	164		162		162	

Notes: * $p < .05$, ** $p < .01$

Table P2: Estimated Effect of 287(g) on Hispanic-White Segregation, by Agreement Timing and Type

	Hispanic-white <i>D</i>	
	(1)	(2)
Early adopter of 287(g)	.014 (.008)	
Late adopter of 287(g)	.031 ** (.008)	
Jail		.022 ** (.008)
Task Force		.021 (.011)
Hybrid		.026 ** (.010)
Post period (2010)	-.058 ** (.003)	-.058 ** (.003)
p-val ($H_0: \beta_1 = \beta_2 = \beta_3$)	.100	.156
County fixed effects	yes	yes
Controls	yes	yes
N of applicant counties	164	164

Notes: * $p < .05$, ** $p < .01$

Table P3: Estimated Effect of Mechanisms Contributing to Effect of 287(g) on Hispanic-White Segregation

	Hispanic Net Migration ¹	White Net Migration ¹	Census Coverage ^{2, 3}	Hispanic-white <i>D</i> ³
	(1)	(2)	(3)	(4)
Implemented 287(g)	-1.166 (2.019)	.027 (.163)	.280 (.145)	.023 *** (.006)
Post period (2010)	-9.085 ** (1.148)	-.330 ** (.093)	-.704 ** (.090)	-.036 *** (.010)
Hispanic Net Migration				.002 ** (.000)
Census Coverage				.004 (.004)
County fixed effects	yes	yes	yes	yes
Controls	no	no	yes	yes
N of applicant counties	164	164	122	122

Notes: * $p < 0.05$, ** $p < .01$; ¹ net migration expressed as number of net migrants per 1,000 expected population; ² Census Coverage is the percentage of the estimated population under (negative values) or over (positive values) in the decennial census enumeration; ³ the smaller Ns in models 3 and 4 are due to Census coverage estimates excluding counties with less than 50,000 people